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A
S T A T E
O F
F A C T S
C O N C E R N I N G

The first Proposal of performing the PARACENTESIS of the THORAX, on account of Air effused from the Lungs into the Cavities of the Pleurae :

A N D C O N C E R N I N G

The Discovery of the LYMPHATIC VALVULAR ABSORBENT SYSTEM of VESSELS in OVIPAROUS ANIMALS.

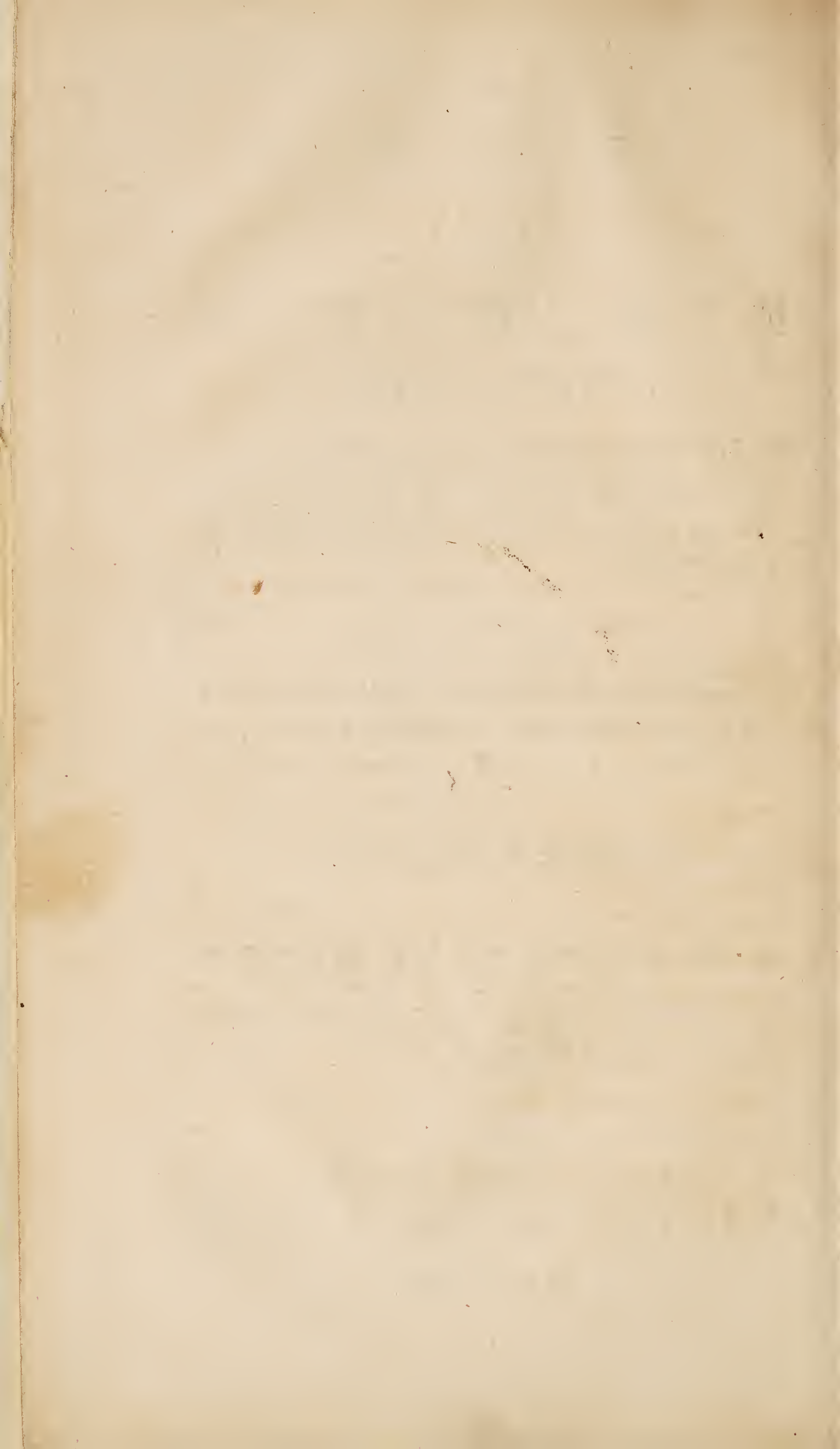
In Answer to Mr HEWSON.

By DR ALEXANDER MONRO,
Physician, and Professor of Physic and of Anatomy in
the University of Edinburgh.

E D I N B U R G H :

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M, DCC, LXX.



STATE OF FACTS, &c.

IT seems necessary to begin with acquainting the reader, that Mr. HEWSON attended my course of lectures on anatomy for the winter 1761-2.

§ 1. Since that time, and so late as 1767 *, Mr. Hewson has proposed the Paracentesis of the thorax on account of air in the chest, as a new improvement of his invention.

In my public courses of lectures, I had advised that operation on the very same account, and had endeavoured, at some length, to shew the advantages that might attend it; not merely from the reason of the thing, but from a case that had fallen under my observation at Berlin in 1757, as well as from a few cases described by authors. I had even explained the method I judged to be the most fit for performing it, not only in the course of lectures Mr Hewson attended, but for several years before that time, and ever since, that is, for ten years successively before his paper was published. I found myself therefore, when it appeared, under the disagreeable necessity of asserting in my lectures my prior right to the proposal of that improvement. In consequence of this, Mr Hewson wrote me a letter, in which he acknowledges that “ he cannot now doubt
that

* See medical observations and inquiries by a society at London, vol. iii. art. 35.

that I had made the observation before him." But the farther particulars of which I think needless to trouble the reader with, since as much as is necessary of these will be sufficiently understood from a following letter of mine in answer to him.

§ 2. Near the close of 1768 Mr Hewson gave an account, to the royal society of London, of the lacteal vessels in different kinds of oviparous animals, entirely as his own discovery. Of this I received the first information in December 1768, in a letter from my brother Dr Donald Monro physician at London; but in which he mentioned nothing farther, than that Mr Hewson had lately read an account, to the royal society, of the lacteals which he had discovered in fowls and fish.

Long before Mr Hewson attended my lectures, I had inflated lymphatic vessels and glands on the neck of fowls, and observed blueish vessels in their mesentery, which I judged to be their lacteals, and had mentioned as such in my lectures. In the year 1765, I had injected with quick silver the lacteals of the sea-tortoise or turtle, and thereby compleated the discovery of the lacteal and lymphatic vessels, or of the general lymphatic system in the oviparous animals.

When I received my brother's letter, I did not know that Mr Hewson had made any experiment on the turtle, and I besides supposed that his other experiments were but recently made. I therefore believed that I had compleated this discovery before Mr Hewson had made any experiment about it; and, in my answer to his letter, I wrote in the following words, to the best of my recollection,

recollection, for I kept no copy of my letter, not supposing it material to do so: "That eight years ago (had I then looked into my book of notes, I would have wrote, nine or ten years ago) I had inflated the lymphatics on the neck of fowls, and had observed, in the mesentery of birds and fish, blueish vessels which I believed to be their lacteals; and that four years ago I had injected with quick silver the lacteals of a turtle, of which I sent him inclosed an engraved figure. I added, that I had mentioned all this in my public lectures, and allowed him to communicate it to the royal society; which he accordingly did."

I was astonished to learn, in a short time thereafter, that Mr Hewson had presented a letter to the royal society, in which he pretended to call in question the truth of what I had wrote, though he had not only never seen, but had never asked from me, any particular account of my experiments, nor acquainted me of his intention.

By that letter, which was wrote in a style very consistent with such a step, Mr Hewson began what he has called a controversy with me. For though, in the very first sentence of a paper he printed in December 1769, under the title of a State of the controversy, &c. he says, "That soon after an account of these (his) discoveries was laid before the royal society, the learned Professor Monro of Edinburgh sent a letter to his brother, in which he asserted that he had anticipated me in them;" yet in fact Mr Hewson very well knew that, whatever my private sentiments might be, this letter did not mention nor assert one syllable about him nor

his

his experiments, the particular circumstances of which I was entirely ignorant of. But, as a letter I received from him, dated July 15. 1769, bears, the account I had laid before the royal society of my own experiments, which I was surely at full liberty to do whenever I pleased, “he construed as a claim to his discoveries.”

Yet, let the reader put himself in my situation, and consider, that I had proposed the paracentesis of the thorax long before and whilst Mr Hewson attended my course of lectures.----That Mr Hewson took notes from my lectures, and was acquainted with many gentlemen who had attended me then and since that time.----Yet that he not only assumed the proposal of that improvement in surgery entirely as his own invention, but, at the close of his paper, rejected, “as a coarse and hazardous method, the thrusting in a trocar”; which however happened to be the very method I had recommended on this account, though I advised incision with a knife in other cases.----And though I had in my lectures asserted and proved my prior right to the proposal of this improvement nine months before I wrote that letter to my brother, and could not doubt, and since have learned, that Mr Hewson was informed of what I said, I found him now, instead of signifying any disposition to do me justice *, broaching another subject, and in like manner passing in silence what he might have

* For the letter I mentioned my having received from Mr Hewson about the paracentesis of the thorax is dated december 31. 1768, and did not reach Edinburgh till some time after my letter to my brother was wrote.

have heard me observe concerning it, when he attended my course, and likewise what, it was not improbable, he might have heard of my injection in 1765, and demonstration in my lectures since that time, of the lacteals in the turtle.---I say, when the reader considers all these circumstances, he will not perhaps think, though I had required an explanation of Mr Hewson's conduct, that I had done so without ground.

As soon, after I was informed of Mr Hewson's letter to the royal society, as my time allowed me, I wrote to him in the following letter an account of my observations on both subjects. And that I might precisely specify the extent of my claim to the discovery of the lacteal and lymphatic vessels in oviparous animals, and thereby avoid all dispute with him, I sent him a literal copy of the several experiments and observations I had made, extracted from a memorandum book, in which, ever since 1756, I have been in use to write down any new observations I learned by experiments, reading, or conversing on a variety of subjects, but more especially on the lymphatic system, of which I have always had in view to give a very particular account. I likewise sent him a copy of the notes from which I lectured before, when, and since he attended my course. And, that the authenticity of these excerpts might be proved beyond all doubt, I laid my memorandum book and excerpts from it, with my notes for lectures, before Dr Cullen and Dr Gregory, whose testimony the reader will find subjoined to my letter.

Copy

*Copy of a Letter from Dr MONRO to Mr HEWSON,
dated Edinburgh, June 8. 1769.*

S I R,

I Received a letter from you in January last, which my time would not then allow me to answer so fully as I wished to do, and I afterwards delayed writing till I should have treated in my lectures of the paracentesis of the thorax, when my letter would, I knew, be still more satisfying to you.

The winter before last, in March 1768, when I came to treat of the paracentesis of the thorax, I explained the several disorders on account of which this operation ought to be performed, and symptoms by which these might be distinguished from each other; and then shewed the methods of operating, in the very way I had done in all my former courses.

I advised that an incision should be made, through all the containing parts, for the discharge of water, pus, or blood; as indeed is recommended by the most eminent writers in different nations, Sharp, Le Dran, and Heister; and mentioned that I had made the operation be done, in that way, on two persons.----But, for air effused, the several causes and effects of which effusion I had before explained, I advised that the common teguments should be cut with a lancet, and that a perforation should then be made into the cavity of the thorax with a small trocar, passed obliquely and worked very cautiously like a drill, and gave several reasons
for

for preferring that method to an incision; and I have since caused it to be practised on one patient with the utmost advantage.

I then spoke to the following purpose, and, so far as I can recollect what I did not write, in the following words.

‘ A paper lately published by Mr Hewson, in the
 ‘ third volume of London Essays, in which the para-
 ‘ centesis of the thorax, on account of air effused, is pro-
 ‘ posed as an entirely new as well as considerable im-
 ‘ provement, obliges me to observe, and to appeal to
 ‘ the notes of many gentlemen who are present, that,
 ‘ ever from my first time of giving courses of lectures in
 ‘ this place, that is, from the year 1758, I have endea-
 ‘ voured to shew the advantages which might attend
 ‘ this operation, and explained the method of doing it.

‘ I am particularly certain as to the date here, because
 ‘ a case I had seen with Dr Meckel at Berlin in the year
 ‘ 1757, of which I used always to make mention here,
 ‘ first suggested to me the usefulness of that operation,
 ‘ and led me to consider the various accidents and cir-
 ‘ cumstances which might give occasion to it. Of this
 ‘ case an account has been published by Dr Meckel in
 ‘ 1766, in the 15th vol. of Mem. de l’Ac. du Berlin;
 ‘ but the advantages that might have attended the para-
 ‘ centesis of the thorax don’t seem to have occurred to
 ‘ the Dr.----Since 1762, I have been still more explicit,
 ‘ from having observed that a certain late writer, Dr
 ‘ Hunter, whom I never before named, lest I should
 ‘ be thought to wish to expose rather than to cor-
 ‘ rect his errors, had overlooked the advantages that

‘ might have attended this operation in a case that fell
 ‘ under his care, of which the history is published in the
 ‘ second vol. of London Effays.

‘ By the date referred to, I mean to say, that I had
 ‘ proposed this operation long before, when, and since,
 ‘ Mr Hewson attended lectures in this place. He *might*
 ‘ therefore have learned it here, or he *might* have
 ‘ learned, from many gentleman who have attended
 ‘ here, that he was anticipated in this improve-
 ‘ ment. I do not however pretend to affirm that Mr
 ‘ Hewson learned this directly from me or from others
 ‘ who had attended me, because I am not sure of it. It
 ‘ is possible he may have been absent from this lecture,
 ‘ or he may have forgot it. I mean only to assert my
 ‘ own right of being before him in proposing this im-
 ‘ provement, and leave it to him to explain his conduct
 ‘ as he shall find best.’

Last winter (in March 1769) I put them in mind of
 what I had said the preceeding winter, because I found
 you had not been informed of the latter part of it; and
 then told them that I had received a letter from you,
 in which you affirm you did not know that operation
 had been proposed by me or by any other person when
 you published, though you found now, by notes taken
 from my lectures, that I had proposed it before it had
 occurred to you, which was only after you had read
 Cheston’s Observations, printed in 1766. And I added,
 I was so far from suspecting your veracity, that I had
 made no particular inquiry on the subject, being fully
 satisfied in having secured my own title as the first who
 had proposed that improvement.

I am sorry to find myself under the disagreeable necessity of entering with you on the discussion of another subject, viz. concerning the discovery of the lacteal and lymphatic vessels in oviparous animals; in treating which also, I shall think it sufficient to assert what belongs to myself.

In 1758, when I was about to write my answer to Dr Hunter, concerning the discovery of the origin of the lymphatics, I made some experiments on living birds (the common cock and hen); but, as I finished this piece in a hurry, my experiments were not so numerous as they perhaps would otherwise have been, especially that, in an experiment 'made March 1758, I remarked a vessel making an arch in the mesentery of a cock between the sanguineous vessels and the guts, which I at first believed to be the trunk receiving the lacteals; but, not being able to inject it on trial, I conjectured to be rather a nerve.'

Accordingly, on re-examining this matter in the following winter, when I came in course to treat of birds, 'April 1759, I observed in a cock what looked like lacteal vessels collapsed and of a blueish colour, which seemed to terminate at the back bone between the testicles.'--- I not only mentioned, but shewed, these to the students; and at the same time said, that Dr Cullen had lately, in January 1759, told me, Mr John Hunter had seen lymphatics on the neck of a swan. And, from the two observations put together, I concluded, that fowls had probably the lacteal and lymphatic vessels like to ours.

Next winter, on 'April 23. 1760, I found lymphatic glands on the neck of a cock, blew them up, and
'lymphatics

‘ lymphatics from them terminating in the ends of the
 ‘ jugular veins.’ After showing these to the students, I
 repeated what I had mentioned the preceeding winter ;
 but now spoke with greater firmness concerning their
 lacteal vessels, as I have always considered the lacteal
 and lymphatic vessels as branches of one general sys-
 tem.

‘ The day following, viz. ‘ April 24. 1760, I discover-
 ‘ ed a whole system of lacteal and lymphatic vessels in a
 ‘ skate, running towards the heart on the left of and
 ‘ above the *vena portarum*; and from these the auricle
 ‘ of the heart was blown up. They are proportionally
 ‘ larger, but have fewer valves than in man.’

I don’t find that in the winter 1760-1 I made any new
 experiments ; so that in this course I treated this subject
 in the same way as in the foregoing. But ‘ I have
 ‘ dissected this year 1761 (in summer) eight skates and
 ‘ about a like number of cods and codlings, but with-
 ‘ out being able to observe by dissection or to inflate any
 ‘ like to lacteal or lymphatic glands. I find indeed that,
 ‘ blowing backwards in the meseraic veins, the intestines
 ‘ and the cellular substance between their coats are in-
 ‘ flated ; but this is no direct proof of branches of red
 ‘ veins absorbing, as these veins may be burst, or the air
 ‘ may have first entered the arteries.

‘ 1761. I have this year too (in summer) dissected
 ‘ twelve full grown cocks fed in different ways, viz. with
 ‘ oats and water, oatmeal and water, milk and bread,
 ‘ oatmeal and madder ; oatmeal and rhubarb ; oatmeal
 ‘ and saffron, and opened them alive.

‘ I observed in the interstices of the great arches of
 ‘ the

‘ the red mesenteric vessels a pellucid network, some
 ‘ part of which seems to be composed of branches
 ‘ sent from a large nerve running parallel with the in-
 ‘ testines, and nearer to them than where the trunk of
 ‘ the mesenteric artery sends off its large branches ; but,
 ‘ although I suspect strongly that there are, here too,
 ‘ numerous lacteals, and I even observe very small
 ‘ knots, which I take to be analogous to our mesenteric
 ‘ glands ; yet I have not observed the above mentioned
 ‘ kinds of food to make any odds in their appearance---.
 ‘ I did in one experiment imagine they were tinged red
 ‘ with the madder ; but I afterwards found that expo-
 ‘ sure to the air produced a like effect : I suppose owing
 ‘ to this, that the blood is coagulated sooner in the
 ‘ veins than in the arteries, and, the animal at the same
 ‘ time struggling to preserve life, the blood is drove far-
 ‘ ther than natural into the arteries.

‘ Besides giving the animals the above kinds of food
 ‘ by the mouth, I injected milk, tincture of madder,
 ‘ and tinctures of indigo and stone-blue, into the inte-
 ‘ stines, at a hole cut in them ; and then kept these in, by
 ‘ tying and sowing up the belly ; and, opening it again
 ‘ after one, two, or three hours, I did not observe more
 ‘ than has been described in the lacteals.---The milk
 ‘ injected towards the anus was passed in three mi-
 ‘ nutes like a greenish water with some whitish cloats,
 ‘ and a second portion injected, was coagulated in like
 ‘ manner.----In the neck, I observe very distinctly val-
 ‘ vular lymphatics which pass through several glands
 ‘ like to our *glandulæ concatenatæ*, and open into the
 ‘ bottom of the internal jugular vein.----If a hole is
 ‘ made

‘ made into the undermost gland, and air blown in, the
 ‘ vein is immediately filled with the air.’

In all those years, viz, in 1758-9, 1759-60, 1760-1, as well as in 1761-2, in which you attended, when I treated of absorbents, I endeavoured, in the first place, to prove that the lymphatics formed an absorbent system, and next considered whether the branches of the red veins assisted in this office. I observed that, if we admitted the facts as they stood when I published in the 1758, it was probable they did assist. But that, as on re-examining the subject, the principal of these alledged facts appeared to be ill founded, I was now inclined to believe the branches of the red veins did not assist.

The very words of the notes I wrote in summer 1759, and on which I lectured afterwards, are

‘ If branches of red veins absorb ?

‘ Arguments for it, are :

‘ 1. Lacteals wanting in fowls and fishes, &c.---
 ‘ 2. Lacteals wanting in some parts of our body.---3. In-
 ‘ jections into red veins enter cavities.---4. Liquors in-
 ‘ to cavities fill veins.---5. On tying red veins, parts
 ‘ become œdematous.’

N. B. The fifth argument is interlined since the year 1760.

‘ But 1st argument removed, and probably 2d--Even 3d
 ‘ attended with difficulties, and 4th not often tried, and
 ‘ very improbable---5th owing to exhalation increased.’

(N. B. The answer to 5th argument is interlined since 1760)

‘ Hence conclude, lymphatics certainly absorbents ;
 ‘ red veins very improbably so.----

‘ Here

' Here shew dif-
 ' ference of com-
 ' mon systems of
 ' body, from the
 ' true one founded
 ' on the above do-
 ' ctrine.

' Fig. 1 is com-
 ' mon system; A.
 ' the artery from
 ' which E. exhalant;
 ' ---L. the lymphatic
 ' vessel----R. V.
 ' the red veins---
 ' and a. a. their ab-
 ' sorbent branches

' Fig. 2. A. artery
 ' --- E. exhalant----
 ' R. V. red vein----
 ' L. lymphatic sys-
 ' tem not continued
 ' from arteries, but
 ' absorbent----- a.
 ' Doubtfull if red
 ' veins have absor-
 ' bent branches.----

FIG. 1.

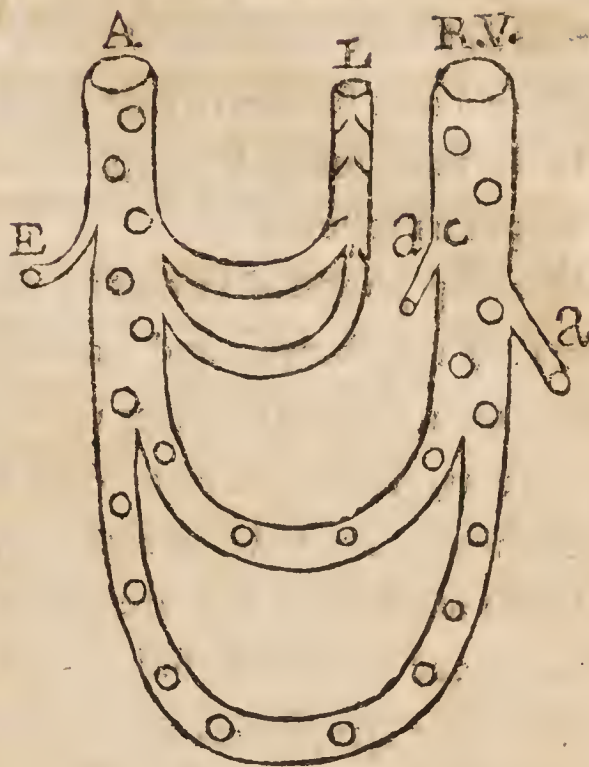
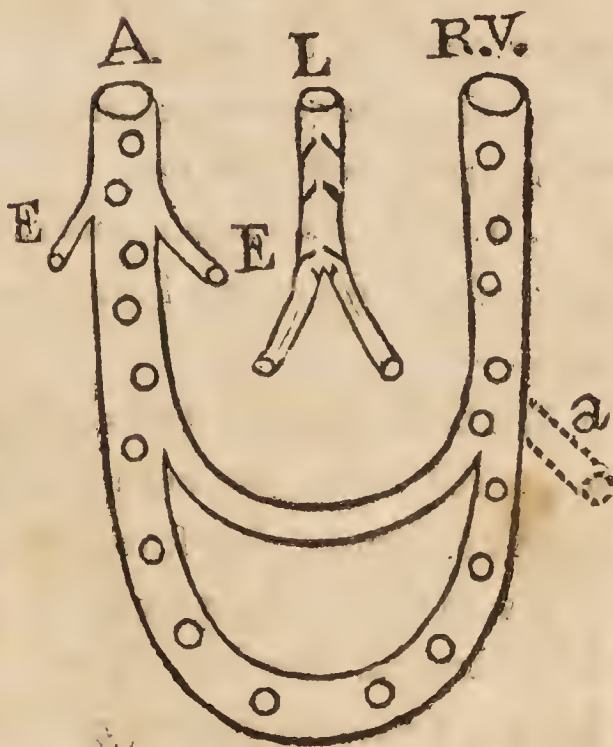


FIG. 2.



' From

‘ From whole, refute prevailing theories---particularly
 ‘ that *vena portarum* absorbs from guts and omentum --
 ‘ That different colour of blood in right and left ventri-
 ‘ cles of heart from vivifying spirit to the latter, &c.--’

In the same summer 1759, I drew out the above notes at length as follows,

‘ Having proved at large that the lymphatics are uni-
 ‘ versally distributed, and universally serve for absorp-
 ‘ tion, we are led to consider whether the branches of the
 ‘ red veins, which are commonly believed to be the sole
 ‘ absorbents, are to be allowed any share in this office.

‘ The arguments that are commonly used for it, may
 ‘ be reduced to the four following heads.

‘ 1. That lymphatics are wanting in fowls, and hence
 ‘ we see that branches of red veins can absorb.

‘ 2. That lymphatics are wanting in some, if not in
 ‘ many places of the human body.

‘ 3. That liquors injected backwards into the red
 ‘ veins pass into the cavities.

‘ 4. Lastly, That liquors poured into the cavities, and
 ‘ particularly the guts, have been observed in the red
 ‘ veins.---

‘ The first argument for the absorption by the branches
 ‘ of the red veins, has been entirely removed; and the
 ‘ second argument almost entirely, perhaps altogether.

‘ As to the third argument, it will be found difficult
 ‘ to distinguish whether a liquor poured into the veins
 ‘ has not previously entered the small arteries, and pas-
 ‘ sed off by their exhalant branches, which may be thus
 ‘ mistaken for inhalants: Or perhaps thin liquors may
 ‘ sweat through the coats of the small veins; or perhaps
 ‘ burst

‘ burst them, without the possibility of observing it.---’

‘ As to the 4th argument, the experiments are few, and not highly probable. So that, upon the whole, the probability now seems to lie against the common opinion; though, till still more experiments are tried, I think it unsafe to pronounce positively on either side.

‘ Let us therefore for the present conclude that the lymphatics are universally absorbents, and that it is not impossible, though improbable, that the small branches of the red veins assist.

‘ In a note shew, that the blood in the cells of the penis is rather impelled into the red veins than absorbed, as injection follows the same rout.

‘ From thus considering the lymphatics, it follows that several theories, which a succession of authors of the highest note have endeavoured to establish and dress out, are void of solid foundation.---To mention two, the most important and generally prevalent.

‘ The *vena portarum* has been supposed wonderfully adapted to secrete bile, by its receiving oily globules directly from the omentum, for conveying which Malpighius imagined he had discovered particular ducts, and by receiving bile itself and putrescent particles, &c. from the cavity of the intestines, by means of the branches of the red veins; whereas the absorbed liquors from both chiefly, and probably entirely, pass into the thoracic duct.

‘ The blood in the left auricle and ventricle of the heart has been thought possessed of several properties different from that on the right side, owing to the air,

‘ or to somewhat from the air taken in by the branches
 ‘ of the pulmonary veins; whereas, in fact, the lymphatic
 ‘ vessels, which are very numerous here, as appears
 ‘ from the extraordinary number of lymphatic glands,
 ‘ carry what they receive to the right side of the heart---
 ‘ And even, should the branches of red veins take in some
 ‘ share of it, that must be subdivided between the pulmo-
 ‘ nary veins and the bronchial, which latter likewise pass
 ‘ to the right side of the heart.

‘ So that whole volumes of reasoning on this curious
 ‘ subject are founded on nothing or next to nothing;
 ‘ and what differences can be remarked, must be chiefly
 ‘ owing to the mechanical agitation of the lungs, and
 ‘ not that the left ventricle is constantly receiving, by
 ‘ means of the pulmonary veins, a fresh supply of vivify-
 ‘ ing spirit from the air drawn into the lungs.----

‘ Here introduce the figures representing the com-
 ‘ mon idea of the œconomy of the body, and the changes
 ‘ which this doctrine makes upon it.’*

When I spoke of the effect of injecting from the
 trunk into the small branches of the red veins, I men-
 tioned experiments I had made about September 1758,
 particularly on the veins of the stomach and intestines;
 preparations of which I then shewed, and still preserve--
 And, when I spoke of the lymphatics of the lungs, I
 shewed an injection of these ending in the thoracic duct,
 which I had made in the winter 1757 8.----

By the by, had Dr Hunter been as fully informed of
 the above, as he seems to have been of other circum-
 stances,

† And to make my doctrine still clearer to the students than it was
 possible to do by words only, I have constantly, from the 1759 down-
 wards, shewn them, in my lectures, the above figures, drawn with chalk on
 a board.

stances, he might perhaps have saved himself the trouble of writing and publishing, so late as 1762, his part of the chapter in his commentary on absorption by veins.

In the summers 1760 and 1761 I wrote an account of experiments, made chiefly on frogs, with opium and other medicines, in order to shew how far these affect animals by acting immediately on their nerves, and how far these affect animals after being absorbed. This paper was read in 1761, to the philosophical society of Edinburgh; and you *might* have heard it read in the anatomical theatre immediately after I treated of absorbent vessels in general, in spring 1762. Though I had never seen any part of the lymphatic system in the frog, yet I was so much influenced by what I have mentioned, that I every where supposed this animal to be furnished with lymphatics.----

In summer 1765, I observed, in the mesentery of a sea tortoise or turtle, blueish vessels, like to those I had before seen in the mesentery of fowls, but larger; which I supposed to be lacteals. And accordingly, after injecting red wax into the mesenteric arteries, and yellow wax into the mesenteric veins, I filled these vessels with quick silver; and found they had valves, but fewer than in the human body. I dried this preparation, which I still preserve; and shewed it in my lectures, next winter, 1765-6, when a very ingenious gentleman, Dr J. F. Palmer, lately physician at Worcester, now at Peterborough, made a drawing of it, from which the figure you saw was engraved.

Explanation

Explanation of the Figure of the Mesenteric Arteries, Veins, and Lacteals, of a Turtle.

G.G.G. Represents a portion of the intestinum ileum.

M. M. M. A portion of the mesentery.

A. A. A, &c. The branches of the mesenteric artery, filled with red wax, and distinguished by transverse strokes.

V. V. V. &c. The branches of the mesenteric vein, filled with yellow wax, and distinguished by longitudinal strokes.

L. L. L. &c. The lacteal vessels filled with quick silver. These have fewer valves than in the human body.

P. L. A plexus formed at the root of the mesentery by the joining and anastomoses of the lacteal vessels with each other.

The truth of all these several facts is unquestionable. Because, in the first place, they were mentioned and demonstrated in my public lectures at the time specified.--- In the next place, because I wrote, into a quarto book I keep for preserving observations and experiments, an account of all the experiments above cited made in 1758, 1759, 1760, 1761, of which the above is a literal transcript, without addition or subtraction. And, from the variety of anecdotes, experiments, and observations, happening before, at, and since these years, with which the notes of the experiments quoted are intermixed,

termixed, their authenticity is placed beyond all cavil.

The exact date of the notes from which I lectured, concerning the question, Whether or not branches of red veins assisted in the absorption, cannot be proved with the same *absolute certainty*, as the account of the above experiments; because they are wrote, as all my other notes for lectures are, on loose papers; but, from the appearance of the paper and ink, it is evident they have been wrote a long time ago: And, as the notes of all my students, who wrote with tolerable accuracy, since the 1759, will be found to tally with them, the date of these is likewise ascertained *sufficiently*.

The lacteals of the turtle, I have already observed, were demonstrated, filled with quicksilver, in my lectures on absorbents in 1765-6, and Dr J. F. Palmer, who made the drawing from this preparation, got his degree of doctor of physic, and left this University, on the first day of March 1766.

It is therefore clearly proved, that I had not only made numerous experiments before you, to discover the lacteals of oviparous animals; but that, in birds, I had, before you, injected part of the lymphatic system; and had a view of vessels in the mesentery, which, from their resemblance to our lacteal vessels, I strongly suspected to be the lacteals of these animals; and that I injected and demonstrated publicly, four years ago, the lacteals in one of this class, viz. the sea tortoise or turtle, agreeable to what I alledged in the letter I wrote some time ago to my brother at London, the contents of which he communicated to the royal society.

The

The reader, who observes that you attended my lectures in 1761-2, that is, after I had made all the above experiments, and when I mentioned all the above facts and arguments, excepting only what related to the turtle, though he should make all the allowance you require for your imbecillity of memory, and should besides suppose it impracticable for you to refresh your memory with your notes, must surely think, that, after the appearance you have made concerning the paracentesis of the thorax, you ought, on your own account, as well as on mine, to have proceeded with the most particular caution and candour on this subject.

As you pretended to have learned, by your own experience in this as well as in the former subject, the strange imperfection both of memory and notes taken from lectures, was it consistent with candour to conclude, that I had neither mentioned nor known more than you found contained in the notes you had seen taken by some young gentlemen? Or, are we to believe that you had entirely forgot *every one* circumstance you heard me mention concerning this subject, as well as concerning the paracentesis of the thorax, but at the same time to take for granted, that no other student could be supposed to have forgot *any one circumstance* concerning these?

You found, it seems, in the notes of some students who attended me in 1765-6, when I shewed the lacteals of the turtle, the following argument, which, though I have no note of it myself, I believe I have sometimes
used ;

used; viz. That, even although lacteals should not have been seen in some oviparous animals, (to please you as much as possible, we shall call these animals *birds*); yet we could not thence deny the existence of their lacteals, because they might terminate soon in the red veins. But, was it consistent with candour, to infer from this, as you have, I am told, done, That it was thence evident I had not seen, as I pretended, what I *believed* to be the lacteals of birds? Is it not, on the contrary, evident, that, from the above experiments, I was myself persuaded that birds were provided with lacteal vessels, and was confirmed in this opinion by having now injected them in one of the same oviparous class, the turtle? Yet, as I had not seen the lacteals in birds filled with chyle, nor shewn them injected, and had only inflated a part of their lymphatic system, I endeavoured by this argument to remove the smallest scruple that could remain in the breast of the most sceptical person, against the general doctrine I was endeavouring to inculcate?

Should we even suppose the above misinterpretation venial, What must the reader think, when he is told, you was informed that a gentleman, who had attended my lectures two years at least before I injected the lacteals of the turtle, that is, nearly about the time you did, declared, he heard me then speak of having *seen the lacteals in fowls*; and yet that you continued to vent this injurious supposition? That is, you must have sunk this material information, since it overturned the whole purport of your story.

To have done: Was it consistent with prudence or
with

with candour, to present a paper to a respectable society, calling in question the truth of my having made experiments and observations concerning the lacteals in oviparous animals, before you had seen or even asked from myself the evidence I could produce of this?

I am

Sir,

Edinburgh,

Your obedient, &c.

June 8. 1769.

Testimony concerning the Facts in the above Letter, by Dr CULLEN and Dr GREGORY, Physicians and Professors of Physic in the University of Edinburgh.

WE, whose names are subscribed, having, at Dr Monro's desire, read this letter wrote by him to Mr Hewson, and having carefully compared with it that part of a Book, bound in Quarto, which contains experiments and observations on the lacteal and lymphatic vessels, and likewise notes for lectures on the same subject, wrote on loose papers, which are referred to: We, in the first place, find, That all the experiments and arguments above quoted are fully and literally copied from the said book and notes: In the next place, from the great number of other experiments

riments and observations on the same general subject, made by Dr Monro, or extracted by him from books, or mentioned to him by others, particularly by gentlemen who studied here from the year 1758, downwards, and who left this university long ago, with which the experiments cited are intermixed; we are satisfied, beyond all doubt, that these were made, and this account wrote of them, at the time specified. With respect to the notes on loose papers, the papers and ink, as well as their connexion with other papers on the same subject, show plainly they have been wrote long ago.

Signed by JOHN GREGORY, M. D.
 WILLIAM CULLEN, M. D.

Edinburgh,

*June 10. 1769. **

Upon

* I am sorry to be under the disagreeable necessity of making the following remarks on some things in Mr Hewson's State of the controversy.

In page 1st, he says, ' I happened to have in my possession an excerpt from some notes taken from his (Dr Monro's) lectures about two years before spring 1762, which contained a similar acknowledgment, viz. that the lacteals in birds and fishes were not then discovered, and that he had sought for them in vain in birds by a variety of experiments-- The above letter shows the Reader, and might therefore have shown Mr Hewson, that this

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' nameless

Upon the whole, it is evident, that, in the years 1758, 1759, 1760, I had discovered by experiments, and mentioned in my lectures, vessels in the mesentery of fowls
and

‘ nameless person must have been strangely mistaken. The
‘ notes of all those students who did me the honour to at-
‘ tend my lectures for any one of three years before Mr
‘ Hewson did so, and to which I appeal, will be found to
‘ prove that I then taught the direct contrary of what he
‘ pretends I did.’

In page 4th, he pretends to say that I don’t, in the above letter, conclude that I had *really* seen these vessels in birds and fish—— What difference Mr Hewson may have discovered, between seeing, and really seeing, I really do not understand. But, if the reader looks back to the experiments I made in April 1759 and 1760, he will find the appearance of these vessels after death really described.

In a note in the same page 4th, he repeats the same thing of Professor Monro’s not being able to conclude that he had really seen those vessels. I believe the Reader will excuse me for not repeating my remark. But he adds, ‘ agreeable to his first assertion,’ as if my first and last assertions had been different. The reader by this time knows that this assertion of Mr Hewson is groundless, and, for the same reason, sees that Mr Hewson might have known it to be groundless when he published it.

Before dismissing Mr Hewson’s paper, I am forced to make one remark farther upon it. In page 4th, where he mentions the excerpts in my letter, he takes pains to signify to the reader by *Italic* characters, that they are from
my

and fish, which I judged to be their lacteals, not merely from their particular colour and appearance in their collapsed state after death, but because, after hearing that Mr John Hunter had seen lymphatics in the neck of a swan, I had found and inflated lymphatic vessels and glands on the neck of the common cock, and traced them to their termination in the jugular vein ; and therefore concluded that, as these branches of the general lymphatic system were certainly discovered, the collapsed blueish vessels I had seen in the mesentery, were the lacteal branches of the same system.

Accordingly, to put this opinion, which, I observed at that time, appeared probable in a degree next to certainty, to the test of experiment, I opened, in summer 1761, a number of fowls, to which I had given food

mixed

my *own* book of notes.---We must suppose Mr Hewson meant this word should convey something more in its italic dress, than it would have done without it, and that he is dissatisfied, and willing to make his reader so, with the book, or with the testimony about it. If the former, I would ask him, In whose book, but in my own, he could expect a particular account of the circumstances I observed in making experiments ? But, to conclude, that for the future Mr Hewson may have no pretence of hinting in any such indirect way what he cannot venture to express in plain English, that book shall, upon demand, be laid, in his presence, before any society or number of gentlemen he shall think proper to appoint,

mixed with various colours, but without finding these in the vessels I had formerly seen in the mesentery. Still, however, as I well knew, that, even in the quadruped, we often don't find the food in the lacteals at the time we expect it; and that in fowls, besides our being much more uncertain of the time the food takes to pass through their several stomachs, the intestines are much less readily laid in view, being not only covered by the breast bone produced, but tied down by a number of membranes; and that, after we have laid them bare, they must necessarily, from their smallness, be much more affected by the cold, than in the large quadrupeds, the common martyrs in such experiments; I was so far from ever holding my disappointment in these experiments as a certain proof that birds wanted lacteals, that I still continued, for the reasons given, to teach, as the most probable opinion, that birds and other oviparous animals were furnished with the lymphatic and lacteal system. But for some years, to wit, from 1761 to 1765, I used to mention my want of success in the above experiments, and hence to observe the uncertainty in this opinion; the decision of which I therefore referred to future experiments. Nay, it is plain, even from notes taken from my lectures, which Mr Hewson has selected from this period, when I spoke with most doubt concerning the lacteals of fowls, that I was then so strongly impressed with the idea of their having lacteals, and I could only be so by the experiments I had made, that I ventured to alledge, that, although we should not

by

by experiments discover their lacteals, we were by no means at liberty to take this as a certain proof that they were wanting, since they might escape observation by their small branches running into the neighbouring veins, instead of joining together as in man.

At last, in 1765, I injected with quicksilver the lacteals of the turtle; and having formerly inflated the lymphatics on the neck of fowls, and seen vessels in their mesentery, which, I then supposed, and now little doubted, were their lacteals; I thought I had completed the discovery of the lymphatic and lacteal vessels in general in the oviparous animals, so far at least as was necessary to illustrate the human physiology; and that it only remained, to trace their particular course in the different kinds by experiments, a labour I once had some thoughts of undertaking, but from which, as from many other subjects of mere curiosity, more necessary and useful inquiries have diverted my attention.

F I N I S.

